Fossil Cryptogams *

I do not propose at present to controvert in detail all the positions taken up by my friend Prof. McNab in his brief communication to your pages on "Fossil Cryptogams" (vol. vii. p. 267), because the time has not yet arrived for doing so. Much more detailed information respecting the subject which yet awaits publication must be had before it can be discussed in a satisfactory manner. I merely wish to avoid leaving the impression, by my silence, that I either admit his supposed facts or accept his inferences. When his paper, to which he refers, was read in Edinburgh, specimens of sections of Calamites of various ages were sent down by me for the purpose of being exhibited to the Botanical Society. This was done by Prof. Dickson, who at the same time expressed his preference for my views over those of Dr. McNab, as is stated in the officially published notice of the meeting in question. Since then I have received a kind letter from Dr. Balfour, who has carefully examined the specimens referred to, and who also expresses a similar conviction. I think that I have unmistakeable proof of the circumferential growth of Calamites, which Dr. Macnab denies, in specimens of large size, and in which the exogenous zone is of great thickness.

Prof. McNab speaks of "the moist nature of the soil in which the Calamites must have grown," as probably causing a different mode of growth in them, to that "circumferential" one which he admits has probably taken place in Lepidodendra, Sigillariæ, and Dictyoxylons; but I beg to suggest that we have no reasons for thinking otherwise than that these plants grew side by side, and under precisely the same physical conditions, hence the "moist soil" of my friend is an assumption. This close association of Calamites with Sigillariæ was demonstrated and commented upon by Mr. Binney many years ago. Dr. McNab further separates Lepidodendron from Sigillaria and Stigmaria, placing them in different groups. When he receives my third memoir in the Philosophical Transactions (which is printed but not yet circulated), he will see how utterly this plan of procedure is opposed to the facts. I contend that Sigillariæ are virtually Lepidodendra, and that Stigmaria is equally the root of both. As to the location of my old, but now abandoned genus, Dictyoxylon, the more I study it the less I feel competent to fix its true place amongst the Cryptogams. But notwithstanding Dr. McNab's idea as to its coniferous affinities, I venture to affirm, from a prolonged study of a cabinet full of specimens, that its woody axis is not one bit more exogenous than those of Calamites and of *matured* Lepidodendra. The fact is that whatever the vessels of these various exogenous woody zones signify, they must stand or fall together. They are either all ligneous or they are all cortical. I think that my forthcoming illustrations of the *bark-structures* amongst the Burntisland Lepidodendra, as well as of our Lancashire specimens, will show that all the elements which Dr. McNab finds in Lycopodium Chamæcyparissus are present, in their proper places, the schlerenchyma of the hypoderm being especially well represented, yet it is precisely this hypoderm with which Dr. McNab believes my exogenous layer to correspond. There is one if not two distinct layers of cortical parenchyma between this schlerenchymatous layer and my ligneous zone, which latter is so magnificently represented in these plants.

The intimate structure of these latter layers, whether we regard the forms and arrangements of the entire woody wedges or that of their component tissues, is so identical in the two cases of Calamites and Lepidodendra, that an active imagination alone can make the one axial and ligneous, and the other cortical. Dr. McNab draws a distinction between vessels representing ("feebly") the fibro-vascular bundles of the living Equisetums, in the Calamites, and the more external portions of each woody wedge, which he regards as representing the hypodermal schlerenchyma of Mettenius. I unhesitatingly avow that there is no ground whatever for this arbitrary separation. He is putting asunder things which have been joined together from the beginning of time. The tissues in question are as identical in their structure as they are uninterruptedly continuous in their arrangement.

Whilst I am thus opposed to Dr. McNab both on questions of fact and of inference, I feel obliged to him for calling my attention to this possible explanation of the facts, even though after a careful study of his views I feel constrained to reject them so far as the interpretation of Calamites are concerned. On the questions relating to Meristem growths, we are much nearer to mutual agreement, and I accept thankfully his admission of the coniferous affinities of Dictyoxylon, not because I am prepared to recognise any specially close coniferous relationships, but because Dr. McNab's idea necessarily involves an admission of the existence of exogenous features in these plants; yet I contend that the Dictyoxylons are neither more coniferous nor more exogenous than most of the other Cryptogamic carboniferous stems which exhibit equally strong proofs of a similar exogenous growth. But I again repeat that we shall not be in a position to grapple philosophically with these problems until all the results of my prolonged researches are published. This is being accomplished as rapidly as my limited leisure admits of. When completed, I shall be quite prepared to enter, if necessary, and in a friendly spirit, upon the entire controversy.

W. C. WILLIAMSON

Owens College

Leaf Arrangement

AFTER reading Dr. Airy's paper on Phyllotaxis (NATURE, vol. vii. p. 343), I cannot see that we are at all nearer than before, any satisfactory explanation as to the inherent cause of it. Let the question be put thus:—If we can conceive, as all will admit, the possibility of leaves being scattered anyhow along a branch, why are they not so, but in some strictly mathematical order? Any disturbance in that order is usually so slight and trivial (due apparently in part to the *conical* nature of the axis, and unequal growth or slight twists; and which thereby cause certain leaves to assume slightly wrong positions), that it does not destroy the fact that they absolutely are arranged, and can be represented, *mathematically*.

In my paper on the angular divergences of the Jerusalem artichoke (Linnean Trans. vol. xxvi. p. 647), I pointed out that two questions might represent all that is required to be solved. (1) That if a leaf be selected as No. 1, then No. 2 lies within a certain arc, viz.:—120°—180° from No. 1, for the ordinary series of fractions, and which it does not transgress—why is this? (2) If we allow that arc—why does the second leaf not assume *any* spot, but is rigidly confined to a certain angular distance from the first?

I cannot think with Dr. Airy that "the way in which all the spiral orders may have been derived from one original order [was] by means of different degrees of twist in the axis." For if we take a piece of round elastic as he describes, with balls fixed according to some spiral arrangement—say $\frac{2}{5}$—then the successive balls will lie at an angular distance of 144°; and if No. 1 be fixed and we twist the indiarubber at No. 2, we may cause it to make a complet rotation if we choose.

If, now, his idea of "twist" be admitted as a *vera causa* of phyllotaxis, we may ask, *what causes the twist to be just so much and no more as to make* No. 2 *pass through* 9° (the angular divergence of $\frac{3}{8}$ being 135), so as to pass into the next $\frac{3}{8}$ arrangement? To say that some such point is a "position of maximum stability" seems to me to give a fictitious importance to the idea of twist, for the expression conveys no really explanatory meaning at all.

Again, to admit that it does not accurately hit the right place, and is in consequence more like Nature, is equally delusive, for Nature is quite accurate enough to be represented mathematically, whereas the positions taken up by the balls must be arbitrary, or at least in proportion to the twist given by the hand—a perfectly arbitrary force? Moreover he appears to overlook the fact that if an axis becomes twisted the *fibres* will be twisted also, but they are not so; the elastic band he adopts would, if it were a pliant shoot, contort the vessels and wood fibres, a condition not obtaining in nature.

Nor can I agree with him in deducing all the members of the series from $\frac{1}{2}$. My experience leads me to infer they are derived from *opposite* leaves, such as one finds in the cotyledons. In the Jerusalem artichoke opposite leaves are frequently succeeded by $\frac{2}{5}$; and this is obtained by the pair of leaves, next above the strictly opposite pair, converging to one side, the next pair do so still more, when it will be found that the $\frac{2}{5}$ arrangement will be henceforth established; the internodes having become more and more developed at the same time.

I strongly suspect the original arrangement to have been whorled and quincuncial. This is at least very abundant, if not universal, in coal plants. The whorls may have subsequently become reduced to fours, threes, and twos or decussate. We see this tendency to symmetrical reduction in many existing plants, *e.g.* stamens and carpels of *Cruciferæ*: *Circæa* as com-

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* We regret that the insertion of this letter has been so long delayed in consequence of the great pressure upon our space.

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pared with *Epilobium*; the stamens of *Geranium* as compared with *Erodium*. Where the reduction has been unsymmetrical, I suspect it has been due to insect adaptation: as in di-dynamous stamens.

As soon as decussate leaves are secured, then we possess the basis for all ordinary leaf-arrangements.

Dr. Airy alludes to non-existing orders, $\frac{1}{4}$, $\frac{3}{7}$, $\frac{4}{9}$, $\frac{4}{11}$, but in the Jerusalem artichoke the secondary series $\frac{1}{3}$, $\frac{1}{4}$, $\frac{2}{7}$, $\frac{3}{11}$, $\frac{5}{18}$, occurs frequently, and arises from the breaking up of "tricussate" whorls in an exactly similar manner to the primary series, $\frac{1}{2}$, $\frac{1}{3}$, $\frac{2}{5}$, &c., arising out of opposite leaves. On the other hand spirals do not easily, if ever, return to whorls. If any one will notice how curiously the above is executed in the Jerusalem artichoke, he will see that there is evidently some power at work in the plant which, as it were, compels the spiral to form, and to form mathematically, will be convinced, I am sure, that a "twist" is very far from being the cause—there being none whatever in the cases mentioned above: and further, when whorls break up, the leaves are at first quite irregular, but they gradually "right themselves," acquire the proper angular divergence, and then form some member of the spiral arrangements to perfection.

GEORGE HENSLOW

Flight of Projectiles

IN reply to the letter of "W. Hope," in NATURE of March 13, I request permission to state that by a *simple* formula, I meant one that would be easily understood. I did not intend the word simple to be taken strictly in its mathematical sense.

It is easy for Mr. Hope to employ symbols to represent the initial velocity, angle of elevation, or any other additional particular he may consider necessary for the solution of my problem.

No one possessing the most elementary knowledge of the theory of projectiles can be ignorant of the disturbing elements to which your correspondent refers, or of others to which he makes no allusion. But these cannot be accurately estimated, and, therefore, must necessarily be neglected in a theoretical investigation. I do not anticipate that they will be found to vitiate the results of theory to the extent Mr. Hope supposes.

In the practical application of the formula for which I have asked, the numerical values of the general symbols, would be the *mean* of carefully conducted experiments. Thus the trifling variations arising from slight differences in the charge, the amount of fouling, or other causes, would be reduced to a minimum. The variations in the force and direction of the wind would often neutralise each other. For these reasons I cannot agree with Mr. Hope in thinking that the calculation would be either "useless or deluding," on the contrary I believe it would be valuable as indicating a mean deflection, about which the experimental deflections would be found to group themselves.

Of one thing I am certain, that it would enable us to bring home to the soldier the great effect of wind in deflecting the bullet, and perhaps it might assist us in dispelling the notion of absurdity which is inseparably associated in his mind with the effort to hit something by aiming at nothing. In accomplishing this one of the greatest obstacles to the development of skill in rifle-shooting would be removed.

If Mr. Hope will kindly supply me with the formula which I have asked for, I can assure him that however lightly he may appreciate the results of his labours, by me, at least, they will be valued, and, I venture to hope, made practically useful. Surely he cannot be in earnest in denouncing all theory which approximates to, but does not exactly accord with practice, as "bastard science, or pedantry." If this *dictum* be sound, I can only say it would be easy to show that a great deal of the science of our day, gunnery science in particular, is spurious.

General Didion, a high authority, did not consider my problem unworthy of investigation. In the *Cours Elementaire De Balistique*, he has given a solution which I regret is rather too complicated for my purpose. I should imagine that he would be the last person to expect his theory to afford more than a rough approximation to the results of practice. Hence I conclude that in publishing his calculation for the benefit of the French army, he could have had no conception that his science was "bastard science, or pedantry," and must have been unconscious what a "mischievous unpractical pedant" he was.

ROBERT REID, Sergeant-Major

School of Musketry, Hythe, March 17

Deep Sea Soundings near the Equator

THE following extract from a letter of the captain of the school-ship *Mercury*, occupied at present in taking deep-sea soundings under the orders of the Board of Commissioners of Public Charities and Correction of New York, has been sent to me by General Bowen, of that Board, who takes much interest in the subject. It will doubtless be gratifying to many of your readers:—

"Our Casella-Miller deep-sea thermometer worked admirably. This beautiful instrument stood the test at a depth of 2,040 fathoms, two miles north of the Equator, in longitude 22° 16′ W., when it indicated a temperature of 35° F.; at 1,000 fathoms 38°; at 400 fathoms 41°; at 300 fathoms 44°; at the surface 81°; in the air 80°.

"On our track from the Canary Islands to Rio we found the temperatures at uniform depths to vary about 2°. Our specimens of the bottom from the volcanic region differ in every respect from those obtained in other parts of the ocean."

JOHN WM. DRAPER

University, New York, March 6

SURVIVAL OF THE FITTEST

THE doctrine of the "survival of the fittest" must be strangely understood in some quarters. The American papers report Prof. Agassiz as having expressed himself in this wise at a recent meeting of the Massachusetts State Board of Agriculture, of which he is a member:—"I do not know how animals originated; a brilliant imagination that of Darwin; a very necessary faculty in the scientist. The sense I know too well to misquote him. Hasty generalising of observation is Darwin all over. Natural selection is out of generation. Natural necessity, what is it? Do we find that only the strong beget families? *Observe plants at the foot of the White mountains, where are large trees, and so up to the summit, where they are mere shrubs.* The weak may and do survive as well as the strong. Ignorance lies at the base of the discussion."

Probably no one naturalist, however eminent, can be expected to know everything, or even all simple things. Can it be possible that Prof. Agassiz supposes (as his argument seems to require) that the dwarf trees in question grow and survive near the top of the mountain, *notwithstanding* they are not the fittest, rather than *because* they are the fittest, for the conditions? And does he conceive the doctrine of natural selection to be founded upon some idea of an abstract fitness, irrespective of the conditions, and not upon the survival of the fittest under and in consequence of the conditions? Surely the argument brought against the doctrine is a good illustration in its favour, only an extremely simple and elementary one.

We never could quite comprehend why Prof. Agassiz should give himself so heartily and persistently to the work of demolishing the doctrine of the derivation of species, in all its forms, considering how large and honourable a part he has himself taken in laying the foundation upon which the modern doctrine has been built. Of these foundations none is stronger than the capital one, generally supposed to be established by him, that the succession of species in time corresponds mainly with that in systematic rank, and is also somehow paralleled in the development of each individual of the higher ranks. So that, in view of his continued but unsuccessful efforts to drive the incoming doctrine out of the land, we could imagine him addressing his own important discoveries in the words used by Balak to Balaam:—"What hast thou done unto me? I took thee to curse mine enemies, and behold, thou hast blessed them altogether."

SUB-WEALDEN EXPLORATION.—SECOND QUARTERLY REPORT

A FRESH survey of the Lower Wealden beds in eastern Sussex by the officers of the Geological Survey Department has quite recently been made. The whole dis-

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